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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global mapping of vertical injection profiles of wild-fire emission" *by* M. Sofiev et al.

Anonymous Referee #2

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The paper addresses a very important topic of nowadays research – the vertical injection profiles of wild-fire emission. It presents a methodology to estimate monthly fire injection profiles differentiating between day and night patterns. It also compares values estimated based on this methodology with results from other authors.

Notwithstanding the pleasure reading the paper it was not an easy reading, because the authors don't always present all the needed information and the readers have to do an effort to clearly understand the full content of the work and results. Anyway, the work is very interesting and deserves to be published after some revisions, due to some drawbacks that still need to be solved improving the paper.

One of the limitations of the presented global mapping methodology regards the absence of information from some regions of the world. In fact, the plume top height

approach was evaluated for some regions of the world, according to available data, but this evaluation is still missing some important regions with different fire characteristics. Authors have to deal with this and to reformulate their language assuming that the global map they present has a lot of uncertainties for these regions of the world.

Section 3.2 is not clear enough. It has to be improved clearly explaining the use of VIRS for fire counting several times per day and the use of SEVERI data. The reader has to do a strong effort to understand the relationship you're trying to establish and suggesting.

Moreover figure 2 FRP diurnal variation (total and per pixel) has to be explained and supported. Ignoring vegetation type based on the presented results (figure 2) doesn't look a good assumption. I don't agree with your last section sentence "Owing to similarity of the profiles for different land-use classes (Fig. 2) and foregoing spatial $1^\circ \times 1^\circ$ grid, the extra uncertainty is believed to be small". Can we base our scientific approach on "believes"?

Probably it's my problem, but I didn't understand why using a gap closure and spatial smoothing approach. Can you, please, be clearer about the need of this procedure? If no fires were detected why are you artificially filling in the empty cells? In fact, later you only present results without this step.

Below you can find some more specific comments:

Page 19211 Line 26 – please, specify the information needed by referred methods; the readers need to better understand why your methodology is an added value.

Line 29 – what do you mean by "climatological" injection profile?

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Lines 13-15 – replace section 4 by section 3, section 5 by 4 and 6 by 5-

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Why separating problem statement and problem solution? It doesn't help the reader to understand the approach.

Line 11 – why monthly? If daily data are available why not estimating daily values and based on this higher temporal resolution aggregate to other periods of time.

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Lines 2-4 – please, provide more bases for the assumption of a similar shape of the vertical profile of emission for all fires

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Lines 9-13- please, provide information about the pixel size of the MODIS data and about the temporal resolution.

Lines 22-24 – please, provide spatial and temporal resolution of the used ECMWF data

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Lines 8-10 – this evaluation has to be expanded to other regions of the world that are not considered yet, but they're very important in terms of smoke impacts (e.g. south Europe, Latin America). Before advancing to a global map of vertical injection profiles you need to be sure that all different fire regions in the world have been considered.

Lines 11-15 – please, better describe the type of used data from CALIOPE.

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Line 2 – specify the meaning of "good" plume height retrieval; it important to know the type of fires included in the dataset? Are they large fires mainly? Can't you provide information about the area burnt by each fire? I know you want to be independent from this parameter, but it is important to know the type of information included in the dataset.

Line 10 – the sentence "Eqs. (1)–(2) can be used over the whole globe" is too strong.

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South European fires or Latin American fires were not included in the dataset and they also have to be testes before generalising the use of Eqs. (1)-(2) to the whole globe.

Line 15 – "their product" means the product of what? Also FRPtotal (total regional) needs a better explanation.

Lines 18-19 – this sentence is not clear enough, please try to improve it.

Line 25 – specify the meaning of "LEO"; this diurnal cycle analysis seems to be based on data from the equatorial region. Can you generalise these results to the whole globe?

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Line 10 – specify SEVERI domain

Table 1 – needs a better explanation

Figure 1 – the variation of FRP, total and per pixel, for the different types of vegetation has to be explained. It is strange to see such a different behaviour between total and pixel FRPs for each type of vegetation. Temporal variation of FRP per pixel for forest shows some peaks that have to be explained.

Lines 9-18 – see my comments above

Line 20 – Why do you call your approach as a bottom-up?

Figure 3 – Even knowing that's not the purpose of the paper I would like to see a quantitative evaluation of the number of recorded fires by comparison with country provide information. At least for some areas of the world this information is available.

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Section 3.3 – please see my doubts above.

Line 19 – what do you mean by "particularly low and particularly high fires"? Please, better introduce Figure 4 contents.

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Figure 4 – I would prefer a comparison between night and day injection heights (for instance for 90% of mass) instead of comparing 50 and 90% mass injection heights.

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Section 5.1 – the identified problem of using FRP density for fires with different burnt areas is important and should be solved, even with high fire area estimation uncertainties it would have been better to take them into account, at least for a comparative study.

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Section 5.2

Your rough representativeness approach is too weak. You're just using two years and you don't even describe them in terms of fire characteristics and/or meteorology (for different areas of the world). These two years could not represent at all the typical fire characteristics and a longer time period is needed.

Fig 6 – Something is missing in my understanding of the described approach. Years are different in terms of fire characteristics and behaviour. To obtain a "climatological" injection profile, as mentioned at the beginning of the paper, you have to use more years, two are not enough and trying to quantify representativeness only based on these two years is not scientifically sound.

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Lines 12-13 – you need a reference to support this or you need to show this correlation.

Line 27 – I would avoid sentences like: "In-average, one can use the ABL-injected fraction of 50–60% as a rough estimate." This value depends on the region and on the meteorology.

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Sections 5.4 and 5.5

The attempt to evaluate fire injection heights using results from other authors is appreciated. However, this comparison should be done more carefully and not only as a generic comparison of maps.

The use of CALIOP observations and their potential link to your results have to be better explained/supported.

It is strange to mention several times a table from another pape; if this table's information is so important it should be included in the current paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 19209, 2012.